independent of any consideration of motion. Force was often defined to be that which caused or tended to cause motion; but the theory of the combination and resolution of forces was founded on certain assumed axioms about the properties of forces without further reference to the effect by which force was described. The proof of the parallelogram of forces was to most beginners such a formidable *pons asinorum* that the broad conception that velocities, accelerations, and forces acting at given points were all fully represented by vectors, and that each could be added just in the same way as the vectors which represented them, was not soon grasped by the mind. Consideration of the fundamental principles of dynamics and of the philosophic position of the first law of motion, which at the same time defines the measure of time and states a law of nature, was avoided, and the theory of the motion of matter became a development of the equations of statics.

Thomson and Tait returned to the order of Newton and abolished artifices from the foundations of the science of dynamics. The influence of Thomson and Tait's "Natural Philosophy" on the volume before us is apparent in the first chapter. The proofs of the parallelogram of forces by Duchayla and Duhammel are conspicuously absent, and the fundamental proposition of statics is deduced quite naturally from consideration of the parallelogram of velocities. When it is once admitted that statics should rest on Newton's laws of motion, the appropriateness of a separate treatise on the subject, to include electrostatics and elasticity, becomes questionable. Why should dynamics be divided and a separate treatise be written on that portion from which it is possible to exclude the idea of mass? A book on the analysis of systems of forces or "wrenches" deals with a natural group of propositions, so does a book on attractions, on electrostatics, or the relations of stresses and strains. But we cannot see that it is natural to group those subjects together with the view, as it would appear, that the student should make himself acquainted with them before mastering the dynamics of a particle. Indeed, however we may admire each chapter of Prof. Minchin's work, we cannot help regretting that he has limited his subject-matter by the title of the volume.

At the end of each chapter is an abundant selection of examples—a very necessary part of an educational work on any department of mathematics. It would have been well that amongst these should have been found a larger proportion of examples demanding a numerical answer; the best students show a liability to failure in rapidly dealing with dynamical questions when concrete numbers take the place of the more familiar symbols.

It is not often that a graduate of Dublin University omits to set forth in its proper place the work of a Dublin professor. Any one would have looked with considerable confidence in Chapter X. of Minchin's "Statics" for some account of Ball's theory of screws as a sequel to Poinsot's central axis, but he would be disappointed. As that theory is very instructive as well as exceedingly elegant, the omission is a loss to the student.

Chapter IX. is devoted to friction, and ends with four articles on the friction of a pivot, based on the assumption that the pressure between pivot and footstep is uniform over the surfaces in contact; and in Art. 134 the equation of the tractory is found by a further condition that the vertical wear shall be constant. As a fact, when a pivot has been at work for some time the vertical wear becomes of necessity constant, and thence may be deduced the normal pressure at any point which will not be constant unless the form of the pivot be the tractory. As an illustration we propose the following to our readers: A conical footstep is to bear a maximum load with a minimum frictional moment; show that it should have a hole in the middle one-third the diameter of the footstep. A similar consideration may be applied to ascertain the distribution of pressure between a horizontal shaft and an ordinary bearing.

The book ends with a chapter on stresses and strains and their relation to each other. The examples appended to this chapter will be found most useful to the student; so far as we know he will not find elsewhere such facilities for testing his skill in this department of dynamics. Although we do not think it desirable that the departments of the science of dynamics should be classified for teaching purposes into statics and kinetics so completely as the present volume implies, we can heartily recommend each several chapter for the subject on which it treats, and we hope that Prof. Minchin will produce a work dealing with kinetics, and that when a fresh edition of both is demanded he will weld them into a single treatise on dynamics.

AUSTRALIAN ORCHIDS

Australian Orchids. By R. O. Fitzgerald, F.L.S. Part V. (Sydney, N.S.W.)

THE part of this beautiful and instructive work which has just reached us contains ten plates, illustrations of sixteen species belonging to the genera Prasophyllum, Thelymitra, Sarcochilus, Dendrobium, Pterostylis, Cleisostoma, and Bolbophyllum, all full of analyses, displaying in a very satisfactory manner the forms, disposition, and, in many instances, the development of the reproductive organs; whilst the letterpress is as full as is that of previous parts, of curious and instructive observations on the habits of the species and their modes of fertilisation. Whether, in point of scientific importance, or fulness of illustration, there are few works upon the Orchideæ to compare with this, certainly none at all comparable to it has ever been attempted in a colony. Its only rivals are the magnificent orchideous plates in Blume's "Rumphia," and in his still more beautiful "Orchideæ of the Indian Archipelago." On the other hand, in respect of descriptive matter the works of these two authors widely differ. Blume had to deal with a host of previously unanalysed and unnamed generic and specific forms, which he classified and described in a truly masterly manner, and his works are hence almost purely systematic. The materials for the "Australian Orchids" had been for the most part classified by Brown in the "Prodromus Flora Novæ Hollandiæ," with a skill equal to that subsequently displayed by Blume in respect of the Indian ones, and Mr. Fitzgerald has therefore rightly devoted his descriptive matter chiefly to the "life-history" of the species. As a specimen of this we may quote his observations on *Prasophyllum fimbriatum*:—

"This little flower presents another of the anomalies

frequent in the family. So constantly does the labellum appear to act as a resting-place for insects, that in trying to trace the probable manner in-which they fertilise a species, you naturally look upon it as the platform of the operator; but in this case, should a tiny insect alight upon one of the lips which hang trembling from the flowers, it would meet with a projection resembling the column and in the same position usually occupied by it, but without anther or stigma, being in fact nothing more than the hinge from which the fringed lip depends. This baffling is caused by the flowers being inverted, and the dropping of the labellum in front of them. Such modifications as this are useful in checking the natural tendency to assume that a certain part of a flower is designed to act in a certain way simply because through a long series we find it performing that function, and to show us how a slight change may alter all the results. Here the labellum bars access from the ordinary direction; the lower sepal incloses the column from below; the petals and wings of the column intercept access from the sides, and a prolongation of the anther obstructs it from the end; so that a very small space is left open beneath the labellum in what would appear to be the least likely place for an insect to approach, though from the conformation of the column the intervention of insects seems to be a necessity. After a very careful examination, I came to the conclusion that the most probable method in which this interesting little orchid becomes impregnated is by a very minute insect alighting on the under surface of the labellum and following it up into the flower, the lip giving way to its pressure upwards (by being lifted on the hinge) should the visitor be slightly too large. Would not the chances of the reproduction of this species be improved by the removal of the labellum? This, then, is another instance of a part of a flower, generally of importance, becoming of very doubtful advantage, if not actually detrimental."

Hitherto Mr. Fitzgerald's studies have been confined to the orchids of Eastern Australia, but it is most earnestly to be desired that they will be extended to the southern and western species, as indeed the title of his work implies will be the case.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Ice-Crystals

SINCE the publication of the Duke of Argyll's first communication on "Ice-Crystals" (NATURE, vol. xxi. p. 274), I have been expecting that some of the physicists who have noticed similar phenomena would have looked up the *literature* of the subject.

About thirty years ago I made this class of phenomena the subject of a somewhat careful investigation, and published the results of my researches in the *Proc. Am. Assoc. for Adv. of Sci.*, third meeting, March, 1850, vol. iii. pp. 20-34; also in the *Phil. Mag.*, 3rd series, vol. xxxvi. pp. 329-342, May, 1850. The *former article* is illustrated by several engravings representing the appearances presented by the "exudation of ice" from the foot-stalks of the *Pluchea*. I think that my investigations show that the phenomenon in *plants* is purely *physical*, having no connection with the *vitality* of the stems; and that it is due to the *same cause* as the "protrusion of icy columns" from the ground in frosty weather.

In relation to the *explanation* of the phenomena, I have nothing to add to that given in the above-mentioned paper, except in relation to *two points*, viz. (1), that I did not sufficiently *emphasise* the importance of the fact that the water contained in the *capillary* tubes in the upper stratum of earth is cooled many degrees *below* the freezing temperature; and (2) that, consequently, the congelation would necessarily take place *paroxysmally*.

JOHN LE CONTE

Berkeley, California, April 27

Anchor-Ice

MY remarks on anchor-ice, published in NATURE, vol. xxi. p. 538, have called forth several letters to myself, in addition to the articles on this subject by Mr. Allan Macdougall and C. F. C. respectively, which have a place in vol. xxi. p. 612 and vol. xxii. p. 31 of your journal. I am happy to find that C. F. C. agrees generally with my views, but I regret to have to differ from him when he says that "the original (ice) crystals, if not *heavier than water*, are at least as heavy." Were this supposition true, anchor-ice might as readily form in one part of a stream as in another, and would not require the conditions which I believe to be necessary. These minute crystals have never been seen by me "distributed" pretty evenly throughout the body of water at rest, nor even where there was a smooth, slow, steady current, which would be the case if the specific gravity of the crystal and water were alike.

C. F. C. is right in saying that this ice resembles manufactured "water ices"; it is never, as far as I know, transparent. It also looks like salt-water-ice.

Mr. Macdougall tells us of anchor-ice in Georgian Bay. This at first sight would appear to be incompatible or at variance with my belief in the necessity of a "comparatively swifter current" being essential for this formation, but to those who are familiar with the large lakes of America, the apparent contradiction seems not difficult of explanation.

At Great Bear Lake inexplicable currents of several miles an hour, sometimes running against the wind, are found in many of the narrow and shallow channels separating islands from the shore, making agitation sufficient to disturb the equilibrium of the floating ice-crystals and surface cold water. The same condition of things doubtless obtains in the Georgian Bay, which is the most easterly portion of the extensive and irregularly-shaped Lake Huron.

One remark of Mr. Macdougall's, to the effect that "the anchor-ice in the great northern lakes *floats* at a considerable depth under the surface of the water, and that it seemed to be floating at various depths in water fourteen feet deep," is curious. One way of accounting for this peculiarity may be that when the air becomes detached from the bottom, it not improbably brings up with it stones or gravel; soon afterwards a part of the ice gets separated, thus diminishing the floating-power, until the specific gravity of the compound mass exactly equals that of water, in which condition it might, of course, be found "swimming" at any depth below the surface.

Mr. Macdougall asks, "Does the (anchor) ice form by action of the intense cold of the ground (meaning, I presume, the bottom of the lake or stream), favouring the formation of rasee?"

I do not think that as a rule the coldness of the ground has anything to do with this formation, except in so far as this coldness of the ground, *i.e.*, the stony bottom, is caused by contact with the ice-cold water and ice-crystals, as already mentioned. "Intense coldness" of the ground at the bottom of the middle of a stream can scarcely be caused by abstraction of caloric, through its connection with the supposed colder land on shore, which is usually covered and protected by snow from cold in early winter; also, were this the cause, or one of the causes, the part of the river nearest the shore would first show anchor-ice, which is not the case.

At Repulse Bay flooding of some of the rapids of North Pole River took place when the ice was forming. This we know could not be caused by a greater flow of water, as the lakes supplying the river and all the rivulets running into them were already firmly ice-bound.

These overflows were caused by barriers of anchor-ice, which dammed the water up to the height of two or three feet, until the pressure became so great as to force a passage through the soft but tenacious mass, the portion of which that remained unbroken being now, by the running off of the water, brought into contact with the cold air, soon became frozen hard and solid.

J. RAE

4, Addison Gardens, W., May 15